THE SELF-PURIFICATION OF RIVERS.

BY CHARLES V. CHAPIN, M.D., SUPERINTENDENT OF HEALTH, PROVIDENCE, R. I.

A GREAT deal has been said and written about the self-purification of rivers, and both the advocates and opponents of the theory that rivers readily purify themselves have taken a very firm stand in the matter. The question, however, is not one which is easily settled. A great many factors must be taken into consideration before we can reach a just conclusion. The question of dilution is an important one. A polluted stream may apparently become purified when in reality there is simply a dilution owing to the addition of pure water either from tributaries or from springs. On the other hand a river may apparently continue without showing any signs of improvement, the improvement being masked by the addition of pollution at various places. It is very seldom that in these discussions this factor has been taken into account and more rarely still that actual measurements have been made, for it is no easy matter to do this. In the first place the amount of water entering the stream between the two points where the tests are made must be approximately known, whether it comes from springs or tributaries. Then the character of this addition must be known, for in most cases the water of tributaries contains very appreciable amounts of pollution.

Another difficulty is the collection of samples. It is evident that if samples are taken from different stations on the same day and hour the sample from the lower station will not by any means necessarily represent the water at the upper station. And even if allowance be made for the flow of the river, it would be extremely dangerous to assume that the water passing the lower station at the calculated time was really the same as that passing the upper station. Yet this method of taking samples at different times should in all cases be followed, though it is by no means the invariable custom.

Another point to be borne in mind is in regard to the chemical analysis of the water. Tests should be made upon the water both before and after filtration through filter paper. In estimating the free ammonia we must take into consideration also the oxidation of albuminoid into the free and the escape of the latter into the air. It is very seldom indeed that all these factors have received due consideration. Perhaps the most careful study of the chemical self-purification of rivers has been made upon the Blackstone by the Massachusetts State Board of Health. A large number of samples were taken with much care during a period of several years, and allowance was made for the addition of natural impurities by tributaries. No allowance was made for the addition of sewage impurities by tributaries, but from a study of the chlorine contents it can fairly be assumed that the amount is small. The first sample was taken just below Worcester, which is the point of pollution, and the last below Millville, twenty-three miles below. The average of all the tests showed a diminution of free ammonia from 1728 to 1299 parts, of albuminoid ammonia from 826 parts to 382, an increase of nitrogen as nitric acid from 218 to 457 and a diminution of total nitrogen from 3000 to 2156. There has evidently then been a very considerable improvement as regards the organic contents of the water, and this notwithstanding a slight addition by tributaries not allowed for. How much of this improvement is due to sedimentation it is impossible to say, but the increase of nitrates shows that it is largely at least due to a true oxidation. Other observers on other rivers have obtained different results, as Percy Frankland from the Thames and Ure, but he does not appear to have taken sufficient account of the addition of pollution by tributaries. On the whole the work of the Massachusetts Board is the most thorough published, and from it we can fairly assume that in a river moderately polluted there is in the course of a few miles a very considerable destruction of dissolved organic matter by oxidation, as well as loss of solid particles by subsidence.

The aeration of the water of rivers in falling over dams and natural obstructions has been supposed to exert an important influence in purification, but this supposition is not correct; for while dissolved oxygen is necessary for the working of certain purifying changes, an excess of oxygen, introduced by however so efficient aeration, has been shown not to extend such process. Careful experimenting in the artificial aeration of water by the Massachusetts State Board of Health has demonstrated that it has little or no effect in decreasing the organic ingredients, and Professor Leed's experiments upon the water both above and below Niagara Falls, where a natural aeration is carried on on a most stupendous scale, showed that there was no chemical purification.

But by far the most important consideration when river-water is to be used for drinking is the presence of pathogenic micro-organisms. We know that certain diseases may be transmitted by means of the contamination of water with their specific poison, and it is known that the poison consists of bacteria. Cholera and typhoid fever are diseases of this type. If the intestinal discharges from persons suffering from these diseases find their way into a river and thus reach a public water supply the most disastrous consequences may occur to the users of the water. The vital question is, do these bacteria disappear as the result of natural agencies at work in the river, and if so, how long an interval is required to accomplish this result. It is well known that most pathogenic bacteria do not propagate in ordinary river water, but on the other hand tend to die out. But they die slowly and may live for days and perhaps weeks. Investigation alone can show whether in running rivers there are factors which operate more unfavorably than in confined waters. Observations on the bacterial contents of a river are not readily made, some of the difficulties of chemical analysis holding for biological as well, and observers are not all agreed upon results.

Frankland found in the Thames and Ure that there was no apparent diminution in the number of organisms as the river flowed along. Elaborate tests were made on the River Spree at Berlin by Frank. Specimens were taken at fifteen different stations above, below and in the city and subjected to bacteriological examination. It was shown that although sometimes hundreds of thousands of bacteria were added to each centimetre of water flowing through the city the number was in the course of a few miles reduced to 3000 to 8000, about the same as above the city. It is probable that this result was due largely to sedimentation.

In the Isar, thirty-three kilometres below Munich, the point of pollution, the bacteria were reduced from 15,231 to 2378.

The following is the average result of ten tests on the Limmat, which flows out from Lake Zurich, past the city of the same name.

STATIONS.	DISTANCE.	NUMBER OF BACTERIA PER C. C.
Outflow from lake,	0	225
Station 1,	1.86	1,731
Sewer outlet,	2.175	296,670
Station 4,	2.485	12,870
" 5,	2.796	10,892
" 6,	3.417	5,902
" 7,	5.903	4,218
" 8,	6.214	2,346
" 9,	8.078	2,110

It has been inferred by some from these experiments that rivers can be relied upon to purify themselves or free themselves from disease-producing organisms by the natural flow of a few miles. I do not, however, think that this is a safe assumption. The difficulty in conducting such experiments is so great, and the knowledge that we possess of the conditions of life for pathogenic bacteria in running water is so scanty, that we are not justified in considering that water can be thus purified. Moreover there are many observations which show that rivers are not so purified.

Thus at Providence an epidemic of typhoid fever was traced to a very slight pollution of a large and rather rapid stream three and a quarter miles above the intake of the city supply.

The city of Philadelphia suffers continually from a high typhoid death rate, and this is due unquestionably to the pollution of the Schuylkill River by sewage, much of which contamination takes place many miles above the intake.

The Merrimac River is polluted by sewage at many points along its course, especially at the cities of Concord, Nashua, Lowell and Lawrence. Typhoid fever has for many years been exceedingly prevalent at Lowell and Lawrence, which take their water supply from the river, although Lowell is fourteen miles below Nashua and Lawrence nine miles further down than Lowell. Moreover, when Lowell has suffered from an exceptionally severe outbreak, Lawrence has had the same experience soon afterwards. Newburyport is seventeen miles below Lawrence and takes its water from springs, but two years ago, this supply being low, a pipe was extended into the Merrimac, and soon after an epidemic of typhoid occurred. These failures in self-purification are very instructive from the fact that the river flows so many miles without being freed from disease germs, and secondly because the river is very large as compared to the amount of sewage which enters it.

From theoretical and experimental considerations, and still more from the experiences first related, we must believe that a river once infected with disease-producing bacteria undergoes only a moderate degree of self-purification. What there is, is because the bacteria either settle to the bottom or die. Complete subsidence probably cannot take place in a flowing river, and as from one to two weeks are required to destroy the vitality of certain kinds of pathogenic organisms it can be only very rarely that conditions necessary for entire purification are found. As sewage is always likely to contain disease-producing organisms it follows that a river which receives sewage should be considered unfit to serve as a public water supply. Certainly if in rare cases it may be safe so to use it, we are not yet able to predicate the necessary conditions.

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as a proof of good faith.

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The Editor will be glad to publish any queries consonant with the character of the journal.

Palæolithic Pottery.

As bearing upon the discussion about the so-called *hiatus* between palæolithic and neolithic times, Dr. Brinton has made a statement in his "Notes on Anthropology" (*Science*, March 9, 1894), to which I must decidedly demur. He says, "All must now concede that palæolithic man made pottery, which was long denied him." I suppose that Dr. Brinton relies for this statement mainly upon the authority of the Marquis de Nadaillac, in various works, and especially in his "Manners and Monuments of Prehistoric Peoples," p. 100. But that gentleman is a closet archæologist and not an explorer, and he bases his opinion upon antecedent probability, and not upon personal knowledge, citing certain authorities of at least questionable value. In the single instance in which he makes an assertion upon his own authority he is certainly wrong. After stating that "Evans and Geikie in their turn assert the absence in England of palæolithic pottery, and Sir J. Lubbock energetically maintains this opinion," he adds in a note "But what is the value of categorical assertions of this kind in presence of the fragments of pottery found at different levels in Kent's Hole?" Now, as I have had occasion to say elsewhere, if this statement were correct, it might be regarded as settling the question, for never were investigations conducted more carefully and more scientifically than were those carried on for fourteen years by Mr. Pengelly, at Kent's Hole, near Torquay, on behalf of the British Association. This is what he says in his report made to that body in 1873, p. 213: "The men of the black mould had a great variety of bone instruments; they used spindle-whorls, and made pottery, and smelted and compounded metals. The older men of the cave earth made few bone tools; they used needles and probably stitched skins together; but they had neither spindle-whorls, nor pottery, nor metals." There could not be a plainer assertion than this of the absence of pottery from the more ancient deposits in Kent's Hole.

So, too, Prof. Boyd Dawkins, whose researches in the bone-caves of England are known to men of science the world over, says in "Early Man in Britain," p. 209: "There is no reason to suppose that the cave men used vessels of pottery, since no potsherds have been discovered in any of the refuse-heaps which have been carefully explored in France, Germany, Switzerland and Britain. The round-bottomed vase from the Trou du Frontal, considered by M. Dupont to imply that the art of pottery was known at this time, is of the same fashion as those of the neolithic age from the pile dwellings of Switzerland, and probably belongs to that age. . . . Had the cave men been acquainted with the potter's art, there is every reason to believe that traces of it would be abundant in every refuse-heap, as they were subsequently in those of all pottery-using peoples, a fragment of pottery or of burnt clay being as little liable to destruction as a fragment of bone or of antler."

It is upon these discoveries of M. Dupont that De Nadaillac rests his belief that in Belgium, at any rate, the cave men made a rude pottery, while the mammoth and the cave bear were still their neighbors. But it is a fact that among the fragments of pottery discovered by Dupont in the Belgian caverns were some that had been made upon the potter's wheel; and it is certainly remarkable that "the round-bottomed vase from the Trou du Frontal" was quietly withdrawn from the glass cases of the Brussels Museum ("Matériaux," x., 332; xvi., 124).

Within the past ten years some discoveries made by M. Fraipont, and his co-laborers, in certain Belgian caverns at Spy, Engis and Petit-Modone, have been supposed by some persons to lend confirmation to Dupont's views. But the thorough discussion of these finds by M. Cartailhac in "Matériaux," xxii., 63–78, shows upon how slight a foundation they are based. The most they can be held to establish, if they are proved, is that during the age of the mammoth pottery was invented by one tribe of savage hunters, living in Belgium; that the knowledge of it never spread, and was finally lost, without having been transmitted to the men of the age of the reindeer.

M. Salomon Reinach, in his masterly "Description